

The Council recommend the re-election of the other ordinary members of the Council, with the addition of the gentlemen whose names are distinguished by an asterisk in the following list:—Capt. W. de W. Abney, F.R.S., Sir R. S. Ball, F.R.S., W. H. Barlow, F.R.S., W. T. Blanford, F.R.S., W. Crookes, F.R.S., Prof. G. H. Darwin, F.R.S., Prof. W. Boyd Dawkins, F.R.S., Prof. J. Dewar, F.R.S., *Sir James Douglass, F.R.S., Prof. W. H. Flower, F.R.S., Dr. J. H. Gladstone, F.R.S., Lieutenant Colonel H. H. Godwin-Austen, F.R.S., Prof. O. Henrici, F.R.S., Prof. J. W. Judd, F.R.S., J. B. Martin, F.R.S., Prof. H. McLeod, F.R.S., Prof. H. N. Moseley, F.R.S., Admiral Sir E. Ommanney, C.B., F.R.S., Prof. W. C. Roberts-Austen, F.R.S., *Prof. Schuster, F.R.S., *Prof. H. Sidgwick, *Prof. Schäfer, F.R.S., W. T. Threlson-Dyer, C.M.G., F.R.S., Prof. T. E. Thorpe, F.R.S., *H. Woodward, F.R.S.

Sir H. E. Roscoe, M.P., moved, and Sir R. W. Rawson seconded, the adoption of the report. The motion was adopted.

The Chairman submitted the treasurer's report, which stated that the receipts for the past year were £5081 6s. 3d., including a balance of £1869 brought forward at the Birmingham meeting. The disbursements included the sum of £1186 18s. in respect of grants in aid of scientific research. The balance in hand was £1718. The report was adopted.

SECTION B.

CHEMICAL SCIENCE.

OPENING ADDRESS BY EDWARD SCHUNCK, PH.D., F.R.S., F.C.S., PRESIDENT OF THE SECTION.

IT is, I can assure you, with a feeling of extreme diffidence that I take the chair to-day as President of the Chemical Section at this meeting of the British Association. When I look round me and see the many distinguished men who are prepared to take part in our proceedings I cannot but very strongly feel that the Council's choice might have fallen on a worthier representative of chemical science than myself. Having in the course of my career bestowed more time and attention to technical matters than to purely scientific subjects, and having moreover arrived at a time of life when active participation in work of any kind must necessarily be drawing to a close, you must not expect from me the accurate knowledge of the present state of chemical science and the questions that are at this moment presenting themselves for solution such as would naturally be required from anyone occupying the post which I have on this occasion the honour to hold. The marvellously rapid progress of chemistry during the last twenty years has made it difficult for the most industrious cultivator of the science to keep abreast of the knowledge of the day, and for a *dilettante* like myself one may say it is next to impossible. I confess myself painfully conscious of my defects in this respect, and I shall therefore have to claim the indulgence of the Section should questions arise on which I am unable to speak with authority, or even to discuss with advantage.

Considering, however, how efficiently I am supported by the gentlemen with whom I have the honour to be associated, and to whom I am sure in any case of difficulty I may appeal for assistance, I trust to be able to perform the duties of my office without discredit. I will not, however, trouble you with merely personal questions, which are always more or less tedious, but proceed with the few remarks which I wish to make, and which, if not new or instructive, may perhaps serve to entertain you during the time usually devoted to addresses of this kind.

I think you will hardly expect me, even were I fully competent to do so, to review the progress of chemistry during the last half-century, for the time at my disposal would be too short and the result at my hands, I fear, unsatisfactory. I shall prefer to call attention in a few words to the chemistry of other days as I knew it, and the chemistry of the present time as known to us all, and to point out what I consider to be the chief characteristics of each. I shall then, with your permission, point out a few of the directions in which, in my opinion, the chemistry of the future will probably be developed, and in this undertaking I shall perhaps be more successful than in the other; for to discuss the history of science requires exact knowledge; but in specu-

lating on its future the imagination comes into play, and to imagine is easier than to describe.

When I first entered on my studies, exactly fifty years ago, chemistry could hardly be called a science—it was rather a collection of isolated facts unconnected by any consistent theory covering the whole field. Most of the important elements were known, but of the exact proportions in which they combine together we were ignorant. The law of definite proportion had been generally accepted, but so imperfect were the data then at our disposal that we may say the law was rather taken for granted than proved. The atomic theory of Dalton as explaining this law had also been adopted by chemists; but it is not unlikely that this theory, then in its infancy, might by the vigorous onslaught of a man of Berthollet's acumen have been upset, and we should then have been left entirely without a guide through the bewildering labyrinth of facts. Of any connexion between chemistry and physics there was in those days no question; of any but the most superficial notions regarding the effects of heat, light, and electricity on chemical substances we had no conception. The idea that chemistry could have any bearing on or connexion with physiology or pathology would have been ridiculed as absurd. I cannot think of the then state of organic chemistry without feeling amused. The state of this branch of chemistry could hardly perhaps be called chaotic or rudimentary, for, after all, what had been done had been well done and neatly done, but the assemblage of facts of which it consisted was devoid of systematic arrangement; it resembled a cabinet of curiosities, the components of which stand in no recognizable relation to one another, or a miscellaneous collection of books placed in an orderly manner on shelves, but without any attempt at classification. As to the genesis of organic compounds, what would now be called absurd notions prevailed. I distinctly remember eminent chemists maintaining that no strictly speaking organic body, even of the simplest constitution, could possibly be formed without the intervention of the so-called vital force. The fact, then recently discovered by Wöhler, of the artificial formation of urea from inorganic substances, was considered as something almost miraculous—*i.e.* as a phenomenon the like of which would never again recur. Without, however, entering into further details, I think I may, without fear of contradiction, assert that the main distinction between the chemistry of fifty years ago and the chemistry of the present day consists in this, that whereas formerly the science dealt chiefly with qualitative reactions, it now occupies itself principally with quantitative determinations. To have established the fact that every chemical phenomenon may be represented in figures, denoting either number, measure, or weight, such figures, when once accurately determined, remaining constant and unchanged through all time—this seems to me the crowning glory of modern chemistry. It is the firm establishment of this principle that has transformed the face of chemistry and has changed it from a mere descriptive into an exact science.

In justice to our predecessors it should, however, be remembered that this principle, though more fully developed in our own day, was not for the first time set up in quite recent times. The labours of Dalton, conducted on quantitative lines, were performed in this city of Manchester in the early part of this century. At the same time Berzelius was engaged in analyzing the most important inorganic compounds and establishing the fact, not previously recognized, strange as it may now appear, that every well-defined substance has a definite chemical composition. But going still further back, we come to the alchemists. Now alchemy, if it has any logical basis at all, is founded on quantitative notions as regards matter. All metals, the alchemists said, consist of sulphur, salt, and mercury (these terms signifying not so much elements in the modern sense as qualities) in various proportions; hence their convertibility. Take copper, remove from it a certain proportion of its sulphurous constituent, and add more of the mercurial, and you have silver; repeat the process with silver, and gold results. At the time of which I speak, though much important analytical work had been done by Berzelius, Rose, and others in organic chemistry, though the veteran Chevreul had led the way in placing organic chemistry on a quantitative basis, and the composition of the most important organic compounds—thanks to the labours of Liebig and his method of organic analysis—had been ascertained, still quantitative determinations were not considered of such paramount importance as at present. In fact, scientific thought did not run in that direction, but satisfied itself, for the most part, with the study of qualitative reactions. It was still possible to see

memoirs by eminent chemists containing not a single quantitative determination. Strange as it may seem, two able chemists, Boettger and Schoenbein, were living until quite recently who worked and obtained valuable results without resorting to the balance, the instrument which of all others seems the most indispensable to the chemist of to-day. The balance was indeed universally employed in my younger days, but no other instrument, properly so called, was ever seen in the laboratory. The spectroscope was not yet invented; the polariscope had not come into use; volumetric analysis was still in its infancy. Even the thermometer was but seldom used. What a different picture does the laboratory of the present day present, with its instruments of precision and its various appliances for effecting quantitative determinations of all kinds!

Whether the universal prevalence of, and exclusive attention to, quantitative methods in chemistry has been an unmixed good may be doubted. Who has not run with a weary eye over the long array of figures, the never-ending tables of which some modern memoirs seem to consist, and not longed for some mere description—were it only regarding trivial matters—to relieve the monotony and fix the subject treated of on the memory? That quantitative determinations given in quite precise terms may occasionally be entirely futile may be seen on referring to the history of alchemy. One of the later alchemists professes to have converted 5400 parts by weight of copper into 6552 parts of silver by the action of I part of a metal-improving substance—philosopher's stone (Kopp, "Die Alchemie"). Here we see the quantitative method applied to a purely chimerical process, elaborated from the depths of the experimenter's inner consciousness, and of no value whatever. Much of what is at the present day carefully worked out and presented to the world in numerical form may, like this statement of the alchemist, pass away and be forgotten. This may possibly be the case with the numerous carefully-made analyses of water which we now meet with, and which we would gladly exchange for a few decided qualitative tests of its hygienic properties. In the case of air and water it is not the minuteness of the noxious matter which causes doubts to arise, but the absence of any decided and undoubted chemical characteristics of the impurities present. It is probable that a refined sense of taste, uncorrupted by the luxurious indulgences which civilization has introduced, would be able to detect differences in drinking-water which might escape the attention of the most consummate analyst.

Whatever objections may, however, be entertained to the application of quantitative methods in natural science, to the exclusion of others, it is certain that important results have flowed from their adoption, inasmuch that we seem to have arrived at the conclusion that the expression of quantitative results is the be-all and end-all of science; that all differences are merely quantitative; that there is no such thing as mere quality. The whole philosophy of our age is expressed in this one proposition: All differences within the sphere of our experience are quantitative. It is the basis of Darwinism, if I am not mistaken, and underlies many of our political and social theories. Of course it is a mere assumption if stated generally, for the phenomena that admit of purely quantitative expression are few in number compared with those that do not; but then it is surmised, and with some degree of probability, that the vast region outside the quantitative sphere will in time come to be included within it. The past history of science seems to render this likely in the future. The science of chemistry has so far, however, presented an insuperable barrier to the general adoption of this view, and will continue to do so as long as the so-called elements remain what we now admit them to be—indestructible, immutable, inconvertible. It is possible to denote all the known properties of gold and silver, their atomic weight, specific gravity, hardness, malleability, action towards heat, light, and electricity, in precise numbers with reference in each case to a certain standard; and yet we cannot say that silver minus a little of this, plus a little of that, constitutes gold—the two elements are essentially and radically distinct. Unless we admit with the alchemists that by taking away a little of A and adding a little of B we can convert one metal into another, one element into another, the quantitative method must fall short of its complete development in chemistry. Numerous attempts have, therefore, been made to show the theoretical probability, even if it should not be possible to prove it experimentally, of the so-called elements being really compound bodies, or at least of their containing a basic matter common to all. My predecessor in this chair has endeavoured to show, in the brilliant address

delivered to this Section on the occasion of its last meeting, that the barrier hitherto presented to us by the intractability of our present elements may be overcome, and has adduced experimental illustrations in favour of his views of the compound nature of the elements. Mr. Crookes has called to his aid the doctrine of evolution, which has proved so valuable an instrument in the hands of the biologist, maintaining that the elements, like the species of plants and animals, were gradually evolved by some process of condensation from a primordial matter called by him protyle, each step in the process being represented by a distinct element. This is doubtless taking very safe ground, for if the process of evolution was the same in the inorganic as it is supposed to have been in the organic world, the process can never be repeated, and we shall, therefore, never be in a position to illustrate it experimentally. I may, however, have misunderstood what Mr. Crookes meant to convey, and, if so, must apologize for the dulness of my apprehension. Granting, however, the possibility of our resolving our present elements, were it in theory only, into modifications of one basic material out of which they have been evolved, the question would still remain to be answered, What has caused this primordial matter to be split up into groups and forms having distinct and opposite qualities? and when this question is answered, if it can be answered even in a problematical way, then other questions would arise, until by degrees we should arrive at the confines of physical knowledge and find ourselves in the region of metaphysics, where scientific reasoning ceases, and thinking for scientific purposes becomes unprofitable. Excursions into this region would indeed be very useful if on returning to physical regions we could every time bring back with us an instrument as potent and far-reaching as the atomic theory has proved to be, a theory which still remains the basis of all our reasoning in chemistry; but then the atomic theory has been quite an exceptional instance. Metaphysical speculation, such as the *Naturphilosophie* of the Germans has dealt in, has, generally speaking, been utterly barren in natural science.

I will not on the present occasion dwell on the vast addition made to the number of useful and beautiful substances by chemists during the last fifty years. Their number is legion, and their mere description fills volumes, whereas half a century ago a dictionary of moderate size would have sufficed for the purpose. Among these newly-discovered substances none are more remarkable than the metals rubidium, cesium, thallium, indium, gallium, the existence of which was revealed by the spectroscope, and which, indeed, would have probably remained unknown but for the labours of Bunsen and Kirchhoff in perfecting and applying that instrument.

I must not, however, omit all reference to a department of chemistry which has been, one may almost say, created within the time to which I am referring—I mean that of synthesis. When I began to study chemistry we only heard of analysis; of synthesis, so far at least as regards organic bodies, we only dreamt as a remote and unattainable region. The only instance then known of the synthesis of an organic substance was that of urea by Wöhler. Synthesis was, indeed, supposed to be an essentially vital process effected under the influence of the vital force, and quite outside the sphere of the chemist. Since then what marvels have we not seen? Alizarin and purpurin, the colouring-matters of madder, have been prepared artificially by Graebe and Liebermann, indigo by Baeyer, not to mention bodies of simpler constitution obtained by comparatively less complicated processes. We are honoured to-day by the presence of Prof. Ladenburg, who has succeeded in artificially preparing coniin, the alkaloid to which hemlock owes its poisonous properties; the first natural alkaloid, indeed, which has been obtained artificially. Looking back at what has been achieved, I think we may entertain the confident anticipation that all the most important organic bodies, acids, alkaloids, and neutral substances will, in course of time, be obtained in a similar manner, though of one thing we may be pretty sure—viz. that we shall never succeed in forming any really organized matter as distinct from organic. The term organic matter is in fact only employed for the sake of convenience, and as an expression handed down to us from former days, since so-called organic compounds are subject to the same laws with regard to composition as the bodies which we name mineral or inorganic, but organized matter such as we find constituting the vessels of plants and animals is a different thing. The protoplasm contained in the vegetable and animal cell is something very distinct from the same matter after the death of the organism, but the difference

between living and dead matter is not of a chemical nature. In referring to chemical synthesis I cannot refrain from expressing regret that so little has hitherto been done in the artificial production of minerals with a view to elucidating the processes by which they were formed in Nature, but it is possible that more has been done in this direction than I am aware of, since this is a department of chemistry with which I am not familiar. It is certain that inorganic chemistry generally does not now receive the attention which it formerly did. The exclusive devotion to the chemistry of the carbon compounds which we find in most of our laboratories at the present day may, however, be accounted for when we see the brilliant results to which the study of these compounds has led.

After these few remarks on the development of chemistry during the last fifty years, of which I know a little, it may seem presumptuous on my part in the presence of some of the most eminent chemists of our day, whose opinions must be of infinitely more value than mine, to say anything about the future of our science and the direction it will probably take. Nevertheless, trusting to your kind indulgence, I will venture on some speculations in this direction, which, if they do not instruct the younger members of the Section, may serve to amuse their seniors, and at all events will refer to subjects on which some thought is well bestowed.

As regards the future of chemistry, the question has frequently suggested itself to me as it has doubtless done to others—Will chemical science go on expanding and developing during the next few generations as it has done in the course of the last hundred years? Will discovery follow discovery, and fact be added to fact, until the record occupies not a few volumes only, but a whole library? Will systematic chemistry, *i.e.* the history and description of all possible combinations of the elements, have any limits? I am inclined to answer in the negative. All human institutions pass through the same phases; they have their rise, they culminate, and decay; and I do not see why the science of chemistry should form an exception. Moreover, it is a natural law that whatever develops rapidly also declines rapidly, and the development of systematic chemistry since the commencement of this century has been perfectly unprecedented. I think it probable that in the course of time, at the rate at which we are now progressing, nearly all possible compounds will have been prepared, all the most important chemical facts will have been discovered, and pure chemistry will then be practically exhausted, and will be in the same condition as systematic botany and mineralogy now are. New compounds will now and then be discovered, just as new plants and new minerals now are, but nothing further will be brought to light that will affect the theories at which we shall then have arrived, whatever they may be. All the material with which the science has to deal having then been brought together, what will happen? Will chemical science cease? Will chemists, satisfied with past achievements, cease to work, confining themselves to practical questions and the history of the days gone by? I think not. The science will continue to develop, but in other directions than those previously pursued. The exhaustion of systematic botany has not put an end to botanical science, for vegetable physiology has opened a wide field to the botanist, one that will take a long time to explore thoroughly. To indicate the directions which chemical science will take in its various applications to other departments of knowledge, as, for instance, in connexion with the study of the physical properties of matter, or in elucidation of the chemical processes whereby minerals have been formed, or those through which geological strata have passed in bygone ages, would not be within my competency, as I should have to touch on subjects with which I am not familiar; but I may be permitted to refer by a few words to a subject, with which, by reading at least, I have become better acquainted, and which seems to me to offer a wide field to the investigator who shall come well provided with physical and chemical knowledge to its cultivation. I allude to the processes whereby the substances constituting the various organs of plants and their contents are formed, and those again to which the decomposition and decay of vegetable matter are due; a subject as to which our knowledge is quite elementary, but which, it seems to me, admits of an extension and development of which we have at present not the least conception.

De Saussure, it is well known, first discovered the fact that plants under the influence of light absorb carbonic acid and give off oxygen, the inference of course being that the carbonic acid and the water present are decomposed, the carbon of the former

and the hydrogen of the latter going to form the various organic constituents of the plant, while the oxygen or a part of it is set at liberty and poured into the atmosphere. The facts as they stand are simply these: what the plant requires for its subsistence is carbonic acid, water, nitrogen in some form (presumably that of a nitrate), certain bases—potash, lime, magnesia, iron oxide, and phosphoric acid. Out of these it constructs the whole of its organic frame, its cells and their contents, rearranging the elements of which its food consists in such a manner as to convert inorganic into organic matter, *i.e.* changing bodies in which the affinities of the atoms are thoroughly satisfied into such as contain them in a state of more or less unstable equilibrium, and therefore liable to alteration when their atoms are allowed to act in accordance with their natural affinities. More than this we do not know; our ignorance of the several steps or stages of the process, if there are any such steps, is complete; all that has been added to the general statement just given is mere speculation. Yet it is impossible to remain satisfied with the present state of our knowledge on the subject. Accordingly numerous attempts have been made to bridge over the chasm which separates the inorganic and organic worlds, not indeed to show that the change does not involve the creation, as was once supposed, of new matter—for this was proved long ago—but to exhibit in its details the hidden mechanism which produces it, but hitherto without success. We know that light is essential to the process of assimilation in plants, since this does not proceed in the dark, but this fact does not help us to an explanation, for light in this case is a mere stimulant, and never produces the same or similar effects outside the vegetable organism. Liebig and others have attempted to show that the process of assimilation in plants commences with the formation of some simply constituted body, such as oxalic or formic acid, with the elimination of oxygen, out of which by condensation and further separation of oxygen more complex bodies, such as sugar, fats, &c., are formed; but there is not the slightest evidence at present in favour of this view. The first product of assimilation that is distinctly recognized is starch, a highly complex organic, we might almost say an organized body, which appears at once with all its characteristic properties, like Minerva springing fully armed from the head of Jove. If we are to adhere to the facts so far observed, we must conclude that the plant does not proceed as we should do in the laboratory, beginning with the more simply constituted compounds and advancing to the more complicated, but that the reverse process is the one actually adopted, the supposed intermediate products being more probably the results of retrogressive metamorphosis. This conclusion is, however, so much opposed to ordinary chemical views that one cannot feel surprised at the constantly repeated attempts to clear up the question. There can be no doubt indeed that much here remains to be done and to be discovered.

Intimately connected with this subject is that of chlorophyll, the green-colouring matter of leaves, which is always found wherever the process of assimilation in plants is going on, and nowhere else, and is therefore doubtless an essential factor in the process. What part it plays in this process is, in my opinion, still unknown. Its action is probably in part chemical, in part physical, and this adds, it may be, to the difficulty of understanding it. It is generally supposed that it is chlorophyll which by its direct action on the carbonic acid and water with which it comes into contact leads to the formation of organic matter with elimination of oxygen. But this is, I think, a mere assumption—an error due, like many others, to a mistaken use of terms. The chlorophyll of chemists is simply an organic colouring matter, like alizarin or indigo, but being in the vegetable cell intimately associated with other matters, vegetable physiologists have attributed to the action of one, and that the most obvious, constituent what is really due to the complex, perhaps even to some quite other constituent of the complex. It is impossible to conceive that the chlorophyll of chemists can be endowed with the remarkable and exceptional properties attributed to it by physiologists; it is a chemical entity, nothing more. It may indeed be said that chlorophyll only acts as it is stated to do when inclosed within the vegetable cell, but this merely amounts to saying that its action is not merely chemical, but is controlled by the vitality of the cell, which, I suppose, means the action of the protoplasm. If chlorophyll is the agent whereby the decomposition of carbonic acid and water is effected, how, it may be asked, is the agent itself produced? It does not come from without; the plant must be able to form it in the first

instance. We are told by vegetable physiologists that the Conifere when raised in total darkness from seeds produce chlorophyll. In light or in darkness I am convinced it is the same; the plant forms chlorophyll as a means to an end. What the end is we know; it is the assimilation of carbon and hydrogen to form organic matter. How does the chlorophyll assist in attaining this end?

In propounding a new theory in reply to this question I venture to claim your indulgence, such as has been accorded to some of my predecessors and others who at these meetings of the British Association have been permitted to make statements and use arguments of a novel or paradoxical character, which, if they effect nothing else, at least afford a relief to the usual routine of scientific reasoning. My experiments on chlorophyll have led me to infer that the constitution of that body is much less simple than it is generally supposed to be. I do not mean by this that chlorophyll is a mixture in the usual sense; everyone who has paid any attention to the subject knows that ordinary chlorophyll consists of several colouring matters, some of which are yellow, not to mention fatty matters which are unessential. What I mean to say is this, that the pure green substance, the chlorophyll *par excellence*, does not belong to the same class of bodies as alizarin or indigo, but contains three elements, each of which is essential to its constitution, one being a basic nitrogenous colouring matter, the second a metal or a metallic oxide, the third an acid, the three together constituting green chlorophyll. The basic colouring matter is a body of very peculiar properties; it is the phyllocyanin of Fremy; the metal may be iron or zinc, the acid I will suppose to be carbonic acid. Now the plant having formed its colouring matter, the metallic oxide being present in some form or other, and the carbonic acid being supplied by the atmosphere, all the necessary conditions co-exist for the formation of chlorophyll. The compound is an unstable one; it easily parts with its carbonic acid, giving it up to the protoplasm or whatever the agent may be that effects its actual decomposition under the influence of light. The advantage of this arrangement would consist in this, that the carbonic acid would be presented in a more condensed state to the agent which effects its decomposition than if it were merely contained in a watery solution, but more loosely combined, and therefore more easily accessible than if it were united to a strong base such as potash or lime. The carbonic acid having been disposed of, the other two constituents would be in a state to take up fresh quantities of carbonic acid, and so on. Chlorophyll would therefore act as a carrier of carbonic acid in the plant, just as hæmoglobin serves to convey oxygen in the animal economy. Numerous objections may of course be raised to the theory of which I give an outline; I only throw it out as a tentative explanation, showing that the function of chlorophyll may be, in part at least, chemical, and that we need not suppose it to be endowed with the marvellous and exceptional powers usually ascribed to it. Other and more probable explanations will doubtless suggest themselves when this difficult subject has been more thoroughly worked out. Eventually, too, it will be found, I imagine, that physical forces as well as chemical affinities play a part in this as in every other process of the vegetable economy. In the case of chlorophyll there can be no doubt that the green colour and the peculiar behaviour towards light have something to do with its action, but on this point it is not necessary for the chemist to pronounce any opinion. I may take this opportunity of mentioning the important experiments of Sachs and Pringsheim on the optical properties of chlorophyll in their relation to assimilation in plants, as they are probably not so well known to chemists as to botanists.

What I have said may serve to show that the very first steps of the process whereby organic or organized matter is formed in plants are hardly understood. We understand still less the further steps leading to the production of the more complex vegetable bodies—acids, alkaloids, fatty matters. Granted that we were able to trace the formation in the plant of a compound of simple constitution, such as oxalic or formic acid, how far should we still be from understanding the building up of such compounds as starch, albumen, or morphia? The syntheses so successfully and ingeniously carried out in our laboratories do not here assist us in the least. We know the steps by which alizarin is artificially produced from anthracene; but can anyone for an instant suppose that the plant commences in the same way with anthracene, converting this into anthraquinone, and having acted on the latter first with acid, then with alkali, arrives at last at alizarin? Indeed the plant never contains ready-formed alizarin

at all. What we observe from the commencement is a glucoside a compound of alizarin and glucose, which, so far as we see, is not gradually built up, but springs into existence at once. When we think of the complicated process by which indigo is produced in the laboratory with the various substances and appliances required, and then see how in the minutest seed-leaves of a plant like woad a still more complex substance, indican, is found ready-formed, we stand confounded at the simplicity of the apparatus employed by the plant, and are obliged to confess that we have no conception of the means whereby the end is attained. The same difficulties occur in other cases, and it will therefore probably be conceded that the synthetic process carried on in plants, from the first step to the last, are not in the least understood.

It might be supposed that after all the labour and attention bestowed on the inorganic constituents of plants we should know something of the part played by these constituents in the processes of assimilation and nutrition, but here the obscurity is as great as elsewhere. We know by experiment that certain inorganic matters—potash, lime, magnesia, iron oxide, phosphoric acid—are essential to the growth of plants; but of their mode of action, or of the reason why certain plants require potash salts, others lime, and so on, we know nothing. Phosphoric acid is no doubt an essential constituent of the protoplasm of the plant; but why cellulose, of which the various organs chiefly consist, should require mineral matters, which do not enter into its composition, for its formation and building up is still a mystery.

The department of chemistry which relates to the decomposition of organic and organized matters presents problems almost as difficult of solution as those relating to their formation and building up; that is to say, the phenomena observed do not apparently obey the same laws as those prevailing in the inorganic world. When I began my chemical studies the difference in this respect between mineral and organic compounds was less clearly seen than at present. The conversion of alcohol into acetic acid, the putrefaction of animal and vegetable matter were thought to be simply due to oxidation; they were phenomena, it was supposed, exactly similar to the rusting of iron, the tarnishing of metals, the fading of colours. That a third body was required to initiate and continue the process of decomposition, that organic matter in contact with purified air would remain unchanged for any length of time—was not known nor suspected. I am not quite sure whether spontaneous decomposition—*i.e.* the splitting up of a complex body without the intervention of an external agent—might not at that time have been considered possible. In order to explain the phenomena of fermentation, the decomposition of sugar into alcohol and carbonic acid, for instance, we had only the theory of contact—devised by Berzelius and Mitscherlich, the latter of whom used to expatiate on the subject at great length in his lectures. When this ghost of a theory was laid by Liebig, who suggested an intelligible explanation of the phenomena in accordance with the facts then known, it was felt to be quite a relief, as affording a resting-place—if only a temporary one—for the mind. The brilliant researches of Pasteur, which have thrown so much light on the action of the insoluble organized ferments, I need only refer to, as they are so widely known, even outside scientific circles; and since also investigations such as his cannot be discussed without some reference to biological questions, which cannot be entered on now. I will confine myself therefore to a few remarks on the unorganized or soluble ferments, one of which I had occasion to examine when engaged in investigating madder and its colouring matters. These ferments, the type of which is diastase—a substance found accompanying starch in the seeds of plants—are soluble in water, perfectly neutral, devoid of all definite form, and though apparently inert, able when acting within the sphere in which Nature has placed them to cause changes and decomposition of the most profound character. Their action excludes everything in the shape of vitality, and yet it is as mysterious and unaccountable as anything that the vitality of the organized ferments is capable of effecting. Indeed, in vegetable, and especially animal, organisms they seem expressly intended for the attainment of certain ends necessary for the well-being, or even the existence, of the organism, inasmuch that it has been supposed, with some show of reason, that it is to bodies of this class existing within the cells of organized ferments, but not separable by any means at our disposal, that the changes produced by the latter are really due.

A great deal of attention has been paid to the product and

results of fermentation, but very little hitherto to the *modus operandi* of the ferments themselves, and yet this seems to me to offer a wide field for interesting research, especially in the case of those of the soluble class, which are easily prepared, and can be manipulated in the laboratory like any chemical substance without the tedious precautions and preliminary operations necessary in the case of the organized ferments. In what way, it may be asked, do these soluble ferments produce the effects peculiar to them? Is the action essentially chemical, or is it due to physical causes as well? Is the quantity of fermentable matter acted on by a certain quantity of ferment unlimited in amount, or are there limits to that amount somewhere? Does the ferment itself undergo any change during the process of fermentation, or is it the same afterwards as before and capable of acting on fresh quantities of fermentable matter? When a ferment is replaced by a strong mineral acid, the products of decomposition being the same, is the *modus operandi* in both cases alike, or must a different explanation be in each case sought? These questions have never been satisfactorily answered, and await solution. I know of only one attempt to show what actually takes place during a process of fermentation set up by a soluble ferment.

The experiments of Wurtz (*Comptes Rendus*, xci. 787), on papain, the soluble ferment of *Carica Papaya*, led to the conclusion that the fibrin on which it is made to act combines in the first instance with the ferment itself, the latter after the hydration of the fibrin is completed being again set at liberty, and then able to act on fresh quantities of fibrin. Thus, according to Wurtz, the action is found to be the same as that of chemical agents, properly so called, such as sulphuric acid, of which minute quantities may exert a hydrating action in consequence of the transitory formation of compounds which are constantly being produced and again decomposed.

There is another question referring to these soluble ferments to which in the present state of our knowledge it is impossible to frame a probable answer, viz. why does it so frequently happen that each ferment exerts a specific action, an action peculiar to itself, this being in fact, in the absence of any marked chemical characters, the only means by which they can be distinguished one from the other? Why does one ferment act on starch only, while the function of another consists in the hydration of fibrin, that of another in the decomposition of a glucoside, and so on? In accordance with the explanation of Wurtz, we should say that a specific ferment is one capable of combining only with the body on which it is to act, and with no other. I was led to ask this question when engaged in the examination of the colouring matters of *Rubia tinctorum*. The root of this plant, the madder of commerce, contains glucosides, which, though coloured, are quite devoid of tinctorial power. Nature has at the same time placed in the root a peculiar ferment, which, coming into contact with these glucosides at a certain temperature, effects their decomposition, splitting them up into glucose and true colouring matters. Now this ferment is a body *sui generis*, and cannot be replaced by any other ferment that I have tried; its action is specific. Why Nature should have deposited this body in the recesses of the plant for the express purpose of acting on certain glucosides and forming colouring matters, the object of which, so far as the economy of the plant is concerned, can only be guessed at, is difficult to understand. One is inclined in such a case to revert to the old-fashioned doctrine that some natural processes were devised for the use and delectation of man. It is quite certain in the case of madder that had it not been for its peculiar ferment erythrozoy, the valuable tinctorial properties of the root, which have for centuries been applied in the production of that splendid dye Turkey red, would have remained unknown perhaps to the present day, since the only efficient substitute for the natural ferment is a strong mineral acid, and such acids and their uses were unknown in former days.

I am inclined to think that some of the younger chemists and physiologists of to-day may live to see the time when all the at present mysterious and unaccountable processes going on in the organisms of plants and animals, including those of fermentation, will be found to obey purely physical and chemical laws. To the biologist it may seem derogatory to the dignity of his science to have the principle of vitality, which has so long reigned supreme, dethroned and replaced by hard, unbending law. Such, however, is not the opinion of that distinguished botanist Sachs, who says, referring to this very point:—"Der Organismus selbst ist nur die aus verschiedenen Theilen bestehende Maschine, die durch weitere Eingriffe äusserer Kräfte in

Bewegung gesetzt werden muss: von ihrer Struktur hängt es ab, welchen Effekt diese äusseren Kräfte an ihr bewirken. Es würde einen sehr niedrigen Horizont wissenschaftlicher Bildung verrathen, in diesem Vergleich eine Herabsetzung des Organismus sehen zu wollen, denn in einer Maschine, wenn auch nur von Menschenhänden gemacht, liegt das Resultat tiefsten und sorgfältigsten Nachdenkens und hoher Intelligenz, soweit es ihre Struktur betrifft, und wirksam sind in ihr schliesslich dieselben Naturkräfte, welche in anderer Combination die Lebenskräfte eines Organs darstellen. Die Vergleichung des organischen Lebens mit unorganischen Processen kann nur dann als eine Erniedrigung des ersteren gelten, wenn man so thöricht gewesen ist, die letzteren als etwas Niedriges und Gemeines aufzufassen, während die unbegreifliche Grösse und Durchgeistigung der Natur in beiden Fällen sich gleichartig offenbart" (*Vorlesungen über Pflanzenphysiologie*). The time may be far distant when these views of the great botanist shall be universally accepted; but they will, I think, sooner or later prevail.

The little known territory which separates the domains of chemistry and physiology will, in my opinion, offer a wide and interesting field for research, after that of pure chemistry shall have been exhausted or lost its interest. Most important problems connected with life and its relation to the inorganic world there await solution, and I confess that I am inclined to envy the young investigator who, coming provided with an ample store of chemical and physical knowledge, shall apply himself to the solution of these problems. The pleasures derived from the successful pursuit of such studies belong to the highest and purest that we are able to conceive. I can, however, only repeat what has so often been said before, and what the young man of science should not forget, that a life devoted to research only involves no material rewards; it certainly never secures wealth, sometimes not even honour nor fame. Looked on with indifference or even dislike by the State, the Church, and the public at large, all that the man of science can certainly look forward to at the close of his career is the addition at his hands of a few stones to the vast edifice of truth, and the consciousness of having attained a higher stage of intellectual insight.

You may probably expect me, before I conclude, to make some reference to technological matters, to the various chemical arts and manufactures for which the Manchester district is noted. At the last meeting of the British Association in Manchester a report on the condition at that time of manufacturing chemistry in the South Lancashire district, by Sir Henry Roscoe, the late Dr. Angus Smith, and myself, was laid before the Chemical Section. A similar report showing the progress made in chemical technology since that time would have been interesting. Great changes have taken place during the period that has elapsed, especially as regards the alkali trade, and quite a new branch of industry has been developed, that of the coal-tar colours. A description of these new features of our chemical industry with statistics of production would therefore have been acceptable. The idea of a report had however to be given up on account of the difficulty of obtaining reliable information as to details, and in these matters it is the details only which are interesting, the general features of the subject being well known. It can hardly be a matter for surprise, I think, that our manufacturers, considering the active competition to which they are exposed, and the disadvantages under which they labour in consequence of the exclusiveness of foreign nations, should be loth to furnish information which would benefit their rivals in trade. Several interesting papers on branches of chemical industry by gentlemen well versed in them will, however, be read before the Section, and these will, to a great extent, make up for the want of a general report. In the Chemical Section of our Jubilee Exhibition, too, you will see a very fine collection of chemical products, more extensive and beautiful, perhaps, than any previously brought together, and these will give you a good idea of our industrial activity. It would have been interesting to witness step by step some of the processes employed in the manufacture of these various products, but this, I am sorry to say, must not be expected generally.

To some it may seem that this Jubilee Exhibition shows the manufacturing industry and prosperity of this district at least at their highest state of development; that they are now at their meridian, and in the future are doomed to decline. If this be so—and there are certainly indications which seem to favour this view—it would be well for those whose visits here are only occasional to take especial note of the present state of things so as to be able to compare their impressions when they next visit

us with those now received, since gradual changes in communities, as in individuals, are more patent to casual observers than to those who are always on the watch.

From some points of view the signs of the times are certainly not encouraging. It should not be forgotten that the manufacturing prosperity of this district depends to a great extent on the ample supply of a product which is brought to us at some cost from tropical and semi-tropical countries to be re-exported in the shape of manufactured goods. A political convulsion abroad, and this, unfortunately, is a casualty that may at any time be expected, or even the determination on the part of other nations to starve us out, however short-sighted such a determination might be, might cut off our supplies and disable us permanently as we were partially disabled twenty-five years ago. If to this be added the fact that foreign nations are becoming increasingly hostile and exclusive commercially, we cannot feel surprise at the dismal forebodings entertained and the confident predictions of decline uttered by some who claim to know all the facts. I ought to apologize for alluding to so gloomy a subject on the occasion of this to a great extent festive gathering, but then men of science like to look at a question not only from a hopeful but from every point of view. Fortunately on this question they are not called upon to pronounce any opinion one way or the other.

Should this be the last time that Manchester shall entertain the British Association in the day of its prosperity, I can only say with the German poet—

"Schliesst den Kreis und leert die Flaschen]
Diese Sommernächte feiernd,
Schlimme Zeiten werden kommen,
Die wir Auch sodann ertragen."

Whether in prosperity or adversity I feel sure that this city will always endeavour to entertain its visitors to the best of its ability. On the present occasion I may with confidence on the part of the chemical world of Manchester offer to the many friends from near and far who honour us with their presence at this meeting a most hearty welcome.

SECTION C.

GEOLOGY.

OPENING ADDRESS BY HENRY WOODWARD, LL.D., F.R.S.,
V.P.G.S., PRESIDENT OF THE SECTION.

SINCE I received the friendly intimation from Prof. Bonney your distinguished and able President of last year, that the Council of this Association had done me the honour to select me to occupy the Presidential chair of this Section which he had vacated, I have been greatly exercised as to what subject to choose for the brief address with which it has now become customary to open the session. Not that there is any lack of materials ready to hand for the purpose—on the contrary, the subjects embraced by geology are now so varied and extensive that the effort to focus them in a single mind is ever becoming a more difficult task to accomplish, and demands the literary skill of a Lyell or a Geikie to marshal and arrange them from year to year in a manner suitable for presentation to you at our annual gathering.

Foremost in interest must necessarily be that which relates to our home affairs, and in this I have been most kindly favoured by Dr. A. Geikie, the Director-General of the Geological Survey of Great Britain, who sends me a brief notice of the progress of the Survey for 1886, taken partly from his Annual Report as Director-General and partly from information supplied by the office through the kindness of Mr. William Topley, our Recording Secretary. The following is the statement which I have received :—

The survey of the solid geology of England and Wales was completed at the end of 1883, and the field staff has since been occupied in surveying the drift deposits, making at the same time such revisions of the ordinary (solid) geology as may be necessary. In the north and east of England the drift and solid have been surveyed at the same time. The areas examined in the earlier days of the survey, in the south, centre, and west of England, and in Wales, were done for the solid rocks only.

In order to meet the great need for a general map of England and Wales on a moderate scale, one is being engraved by the Survey on the scale of 4 miles to 1 inch (1 : 253440), and will be issued in fifteen sheets.

A few of the survey memoirs relate to large areas, and give complete descriptions of the formations therein exposed, but most of the memoirs are explanations of special sheets of the map. A series of monographs is now in preparation giving full descriptions of special formations. Mr. Whitaker has charge of that on the Lower Tertiaries; Mr. H. B. Woodward and Mr. C. Fox-Strangways are preparing the Jurassic memoir, the former taking the rocks south of the Humber, and the latter those of Yorkshire; Mr. Jukes-Browne is writing the Cretaceous monograph; and Mr. Clement Reid that on the Pliocene beds.

In Scotland some advance has been made in mapping the important and complicated area of the north-west Highlands. The surveyors there were chiefly engaged between Loch Stack and Ullapool, subsequently completing the area about Durness and Eriboll. The other parts of Scotland now being surveyed are the north-eastern and western side of the Grampians, all south of the latter having been already completed.

Ireland is entirely surveyed with the exception of a small area in Donegal, which will probably be completed this year. This district is of interest from its resemblance to the north-west Highlands, and from the problems which it presents as to the origin of the crystalline schists. The recent discovery of organic remains amongst the Donegal schists adds additional interest to this inquiry.

The publications of the Survey during the past year are as follows: England and Wales, six sheets of the map, two sheets of horizontal sections, three of vertical sections, and six memoirs; Scotland, three maps and one memoir; Ireland, two maps and six memoirs.

The next matter which has arisen since our last meeting relates to our colonies, and comes to us in the shape of a message from the retiring President of the Association, Sir William Dawson, who has embodied his ideas in a letter to the President of the Royal Society (Prof. Stokes), copies of which have been sent also to all the learned Societies. To the former I am indebted for a copy accompanied by a favourable report thereon from the Royal Society of Canada.

As the object of this communication is one in which I am sure we, as Englishmen, must all feel a hearty sympathy, appealing as it does to our patriotism in its widest sense, as well as to our devotion for and interest in the science of geology, I feel I shall not need to apologize for introducing it to your notice here.

We are invited by it to enrol ourselves, as geologists, in a Federal Union, composed of all our brethren at home, in our colonies, and in all the dependencies of the British Crown. Nor are we to stop here, for when this has been satisfactorily accomplished it is suggested that we should invite our English-speaking cousins of the great United States, with whom we are already in such close alliance upon so many objects of common scientific interest, to join our Geological Confederation, and, having thus obtained an overwhelming majority, we are to proceed—without armies or vessels of war—to extend our peaceful conquest over every country on the habitable globe, urging and persuading those countries which have not established geological surveys to do so forthwith, and inviting those which have surveys of their own to join our British Association Geological Union. And when all has been accomplished in this direction our exertions as a confederacy may well be extended to secure the mapping of all those outlying regions of the earth's surface at present imperfectly known or still geologically unexplored.

Suggestions such as these could hardly come at a more fitting and appropriate moment, for are we not now on the eve of the completion of the geological surveys of the British Islands? if such a task can ever be said to be completed which has occupied the attentive study of so many able geologists during the last eighty years or more, and from the very nature of the case must always require additional research and revision.

India, Africa, and our colonies may all hope for future assistance from the many geological students now being trained in our schools and colleges, who may not be required in the near future for home surveys, and must needs go further afield to win their title of admission to the ancient and honourable order of "Knights of the Hammer."

This idea of scientific federation was referred to by Prof. Huxley in his Presidential Address to the Royal Society in 1885, and subsequently by the present President (Prof. G. G. Stokes) in November last.

If we would devise a scheme by which we might from time to